further secular rise of from 0·38 to 0·89 of a scale division. Nothing is said in Joule's paper about the temperatures at which the thermometers had been kept before the readings of the freezing point were taken, but as the later observations—and most of the earlier ones—were made in the winter months, it may perhaps be assumed that the temperatures were nearer 7° than 30°, and that the actual reading on the scale last winter should be taken as nearer 23·51 than 23·00. If this is so the total rise of the zero point last winter would be nearer 13·81 than 13·30.

Prof. Schuster states that "with properly annealed thermometers the secular changes are much smaller than the temporary ones," and that is no doubt true for observations extending over a limited time and with such comparatively large variations of temperature as from 7° to 30°. It may be pointed out, however, that the secular rise since 1879 or 1880 is probably greater than the maximum temporary change recorded by Prof. Schuster, and of course the total secular rise is enormously greater.

It may be true that the secular changes of a thermometer gradually vanish, but it must, I think, be conceded, that in the case of Dr. Joule's thermometer it will be a long time before absolute constancy is attained. There can be no doubt that even now, nearly forty-nine years after the first reading was taken, the zero point is still rising, and it does not appear to me to be very improbable that during the next fifty years there may be a further rise of two scale divisions, the amount calculated from the purely empirical formula which I have suggested.

Sydney Young.

University College, Bristol, February 20.

Foraminifer or Sponge?

Under the above heading in last week's Nature Dr. Hanitsch briefly draws our attention to Mr. A. Goës' report on the deep sea organisms procured by Prof. Agassiz in the American tropical Pacific, which he describes as Arenaceous Foraminifera, with the name *Neusina Agassizi*.

As it was from me that Dr. Hanitsch received the specimens he describes, which I had after a personal conversation on the matter sent him, for his opinion as to their relation to true sponges, I venture to send some further observations on these interesting forms.

Dr. Hanitsch is, I believe, quite right in referring Mr. Goës' Neusina to Prof. Haeckel's *Stannophyllum zonarium*, as described in his report on the *Challenger* deep-sea Keratosa. But while admitting my admiration of Prof. Haeckel's wonderful production on the *Challenger* specimens, I do not agree with him as to their being true Keratose sponges.

My conclusion is based upon the examination of nearly the whole *Challenger* collection, and in not one species could I find the slightest trace of any of the flagellated chambers characteristic of sponges.

Prof. Haeckel accounts for the absence of this important feature through the bad preservation of the specimens. Yet he describes the most delicate parts of a commensal Hydroid in full, and was able to observe amœboidal cells, and the granulated sarcode bodies peculiar to all bottom living Foraminifera.

If, however, the forms described by Prof. Haeckel prove after all to be true Keratose sponges, the present state of our knowledge does not justify their separation from such recognised genera of Foraminifera, as Masonella, and Syringammina of the late Dr. G. Brady; Technitella, Haliphysema, and Marsipella of Canon Norman; or *Hyperammina palmiformis*, described by myself from the Faröe Channel, all which forms have the power of forming siliceous and chitinous skeletons.

Without going into further detail here it will be readily understood that I quite agree with Mr. Goës in placing these organisms among the Foraminifera, although it would have been better had he given us a clearer and more detailed description of his Neusina.

I had hoped to have published my personal observations on these most interesting organisms, but circumstances have prevented me doing so up to the present.

I for one would be glad if Dr. Hanitsch would give his opinion as to their supposed sponge structure, which he has not done in his previous letter.

F. G. Pearcey,
Late of the *Challenger* Expedition and Commission.

Owens College Museum, Manchester.

Colonial Meteorology.

On p. 363 of your last number your reviewer of the "Year-book of the Imperial Institute," after remarking that "climate certainly deserves better treatment," continues:—

"We do not think space would be wasted in giving the mean monthly temperatures and rainfall for the average year and for two extreme years, at a few representative stations in the larger colonies. This information cannot indeed be found in any existing books, but must be worked out from original records, which exist abundantly, and are rarely made available to practical workers."

I am afraid that the reviewer does not always read Nature, for you, sir, have on several occasions noticed my efforts in this direction, efforts which have gone on uninterruptedly for twenty years. As, now that you have taken the matter up, it is not improbable that some of the funds lavished on the Imperial Institute may be devoted to the subject, and my small organisation be swamped or superseded, I hope that you will, in justice to the directors of the various Colonial observatories who have helped me for so many years, and as some consolation for the entire ignorement of our organisation by your reviewer, allow me to give its history in the fewest words possible.

In 1873 I determined to try to publish monthly a table giving the principal climatic data for each synchronous month at widely spread stations over the entire British Empire. The leading idea was identity, so as to ensure comparability. I therefore prepared some blank forms and sent them with a circular letter to about twenty of our leading Colonial meteorologists. Every one without exception promised to help, and it says much for colonial climate to add that during the subsequent twenty years not more than five or six of my original correspondents have passed away.

During the period occupied in the transit of my request and of the replies thereto, I wrote a series of short articles pointing out the leading features, and as far as practicable the mean values, for the various stations, so that when we began publishing the monthly values, the departures from the mean could be recognised. These articles and the tables themselves from 1874 to 1881 appeared in *The Colonies* (subsequently *The Colonies and India*). When in 1882 that paper passed into other hands, the proprietors declined to publish the tables, and I began to insert them in the *Meteorological Magazine*, where they have appeared regularly month by month for the subsequent thirteen years. At the close of each year an extra table is given with a summary of the results for the year, and Nature has often done me the honour of quoting portions of these summaries.

I enclose copy of our last table, and though I know that to reproduce it would be to make a somewhat large demand upon your space, I feel that the work (wholly unpaid, be it remembered) of my Colonial friends during these past twenty years, claims some consideration and some recognition. You will see by the signatures that the authorities are the highest attainable.

G. J. Symons.

62, Camden Square, N.W., February 17.

Ozone.

With reference to a paragraph in Nature, p. 373, on observations of ozone in the atmosphere, and the paucity of observers and records, I may be allowed to state that I have collected sets in the North Atlantic and Pacific Oceans and Mediterranean. These have been taken by officers of the Royal Navy and mercantile marine at sea, and some of the records have been tabulated, and may be communicated to some society in due course. Moffatt's papers, made by Negretti and Zambra, have been used throughout, so the observations are all uniform and comparable.

W. G. Black.

Edinburgh, February 19.

LION-TIGER AND TIGER-LION HYBRIDS.

The Council of the Royal Zoological Society of Ireland entertain some hope that it will be possible to produce in their Gardens examples of hybrids or cross-breeds between the two largest species of cat, namely, the lion and tiger.

That such hybrids have been produced is a matter of historical record, and as the writer is particularly interested in the success of the experiment now in progress in the Dublin Gardens, where over one hundred lion cubs have been successfully reared, he thinks it desirable to record all the details which he has been able to collect on the subject.

So far as can be ascertained the only lion-tiger cubs, as they have been called, which were ever produced belonged to several distinct litters by different parents, perhaps, but in the same menagerie—that of F. Atkins, of Windsor.

The father of the first litter of these cubs was a lion bred in Atkins's menagerie, the head-quarters of which were at Windsor. The mother was an imported tigress. From Griffith's account ("Animal Kingdom," vol. ii. p. 448, 1827) it would seem that the lion and tigress were about two years together, in the same cage, before any issue appeared. The first litter, consisting of three cubs, was born at Windsor on October 17, 1824—being the result of a particular intercourse which lasted for ten or twelve days in the beginning of the previous July. The cubs were shortly afterwards exhibited to his Majesty, who, according to the showman's own handbill—a copy of which has been lent to me by Dr. William Frazer—christened them lion-tigers. The lion died six weeks afterwards, and the cubs, as related by Griffith, were fostered by several bitches and a goat, and it was expected would attain to maturity; but although there is no clear intimation as to the exact date when this was written, the figures of the cubs accompanying the account are said to represent them at the age of only about three months. It is stated by one writer, however, that they did not attain to maturity ("English Cyclopædia Nat. Hist.," vol. ii. p. 763, art. "Felidæ," 1854).

The next litter was born at Edinburgh on December 31, 1827, according to Atkins's showbill and Sir William Jardine's works.[1] There were two cubs, and it would seem that they were exhibited together with, and therefore probably reared by, the mother, in the same den; but whether she were the same tigress as the mother of the previous litter is not clear.

They were seen by Sir William Jardine in September, 1828, and his figures may have been taken from them; but it has some resemblance in details, though not in general pose, to the figures published by Griffith of the 1824 litter. It would seem that Sir William was under the impression that it was these very cubs which were subsequently exhibited together with their parents in the same cage in the autumn of 1829. But there is a difficulty in accepting this conclusion, because the stuffed specimens of these two cubs still exist—one in the British Museum (Natural History) and the other in the Science and Art Museum, Edinburgh. I have recently had opportunities of examining both, and I should be inclined to think that the cubs were not *more* than about nine or ten months old when they died. So that either the cubs seen in 1829 were born subsequently to December 31, 1827, or the stuffed cubs just referred to must have been born previous to that date. That the cub in the British Museum was presented by J. Atkins, of Windsor, is attested by Dr. Gray's "Old List," page 40, which, through the courtesy of Dr. Gunther, I have been able to consult.

That the specimen in Edinburgh was one of those born in 1827, and figured by Sir William Jardine, is, indeed, stated in the "English Cyclopædia," which adds that the cubs of that litter died young. Hence, it seems most probable that the cubs seen in the autumn of 1829 belonged to a subsequent litter, as has been suggested above. Further, Mr. J. G. Robertson, formerly of Kilkenny, has informed me that he saw a lion, tigress, and their three hybrid cubs in one cage in Kilkenny, where they were brought by a showman about the year 1832. They were the sole stock of the show.

Accordingly, it seems that besides the definitely attested births of the years 1824 and 1827, there were also, probably, some others. One of the accounts states that there is no great difficulty in promoting the union of the two species.

Besides the cub already referred to as having been presented to the British Museum by J. Atkins, I have also been shown by Dr. Günther unmounted skins of two reputed hybrid lion-tiger cubs, which are said in Dr. Gray's list to have been purchased from a dealer named Mathur, in 1842. They cannot, I think, have survived more than two or three days after birth, and their markings are too indistinct to justify any special description, particularly as their parentage is not more definitely attested. But it is of some importance to place on record here what is said as to the markings of the cubs first referred to. The specimens in the British and the Edinburgh Museums are both somewhat faded. In Gray's list the former is thus described: "Hybrid cub between lion and tigress; yellow; back slightly waved; limbs and tail banded with black."

Sir William Jardine merely says the general colour was not so bright as that of the tiger, and the transverse bands were more obscure.

Griffith describes the cubs he figured as follows:—

"Our mules, in common with ordinary lions, were born without any traces of a mane, or of a tuft at the end of the tail. Their fur in general was rather woolly; the external ear was pendant towards the extremity; the nails were constantly out, and not cased in the sheath, and in these particulars they agreed with the common cubs of lions. Their colour was dirty yellow or blanket colour: but from the nose over the head, along the back and upper side of the tail the colour was much darker, and on these parts the transverse stripes were stronger, and the forehead was covered with obscure spots, slighter indications of which also appeared on other parts of the body. The shape of the head, as appears by the figures, is assimilated to that of the father's (the lion); the superfineness of the body on the other hand is like that of the tigress" (p. 449).

Prof. R. H. Traquair, F.R.S., keeper of the natural history division of the Edinburgh Museum, has kindly had a photograph taken of the specimen above referred to prepared for me, and the transverse markings are distinctly visible in this picture.

I am tempted to conclude this record with an extract from Atkins's somewhat quaintly-expressed handbill, which does not bear any date, but probably belonged to the year 1828. The greater part of the bill consists of a long poetical description of the family with "a tigress their dam, and a lion their sire," and of the numerous distinguished persons who had paid them a visit. The following prose portion will probably be sufficient to extract from what is possibly one of few still existing copies of the handbill.

"ATKIN'S IMMENSE MENAGERIE.

"Wonderful Phenomenon in Nature.

"*The singular, and hitherto deemed impossible, occurrence of Lion and Tigress in one den.*

"Cohabiting and producing young, again took place in this menagerie, on the 31st of December, 1827, at the City of Edinburgh, when the royal tigress brought forth two fine cubs!! And they are now to be seen in the same den with their sire and dam. The first litter of these extraordinary animals were presented to our most gracious Sovereign, when he was pleased to express considerable gratification, and to call them lion-tigers, than which a more appropriate name could not have been given. The great interest the lion and tigress have excited is unprecedented; they are a source of irresistible attraction, especially as it is the only instance of the kind ever known of animals so directly opposite in their dispositions forming an attachment of such singular nature. Their beautiful and interesting progeny are most admirable productions of nature. The group is truly pleasing and astonishing, and must be witnessed to form an adequate idea of them. The remarkable instances of subdued temper and association of animals to permit the keeper to enter their den, and to introduce their performance to the spectators, is the greatest phenomenon in natural history."

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[1] "The Menageries, Quadrupeds," Sir William Jardine (2nd edition), vol. i. pp. 191, 192. 1830.

V. BALL.

OBSERVATIONS OF ATMOSPHERIC ELECTRICITY IN AMERICA.[1]

THE meteorological official of the United States known as "The Chief Signal Officer" has sanctioned the publication of this voluminous report of 320 quarto pages, embodying the result of a widespread photographic record and direct reading of atmospheric electrometers carried out under the auspices of the United States Government during the years 1884 to 1888, with the immediately utilitarian object of ascertaining how far it was possible to use electrical indication in weather prediction. As Mr. Mendenhall says, "No studies or investigations which did not bear upon this question were [considered] proper or allowable."

Although thus limited in scope the actual observations made and here recorded can hardly fail to be of service to future investigators into this obscure subject.

The report begins with a historical introduction, in which it is admitted that electricity was first purposely drawn from the clouds in France by Buffon and D'Alibard about a month before Franklin tried his already projected experiment; and that de Saussure was one of the first to obtain fairly quantitative results and to detect a diurnal period.

Volta "hit upon the capital device of a burning match" to replace the previous feeble collecting devices such as a bullet and wire shot up into the air. But nothing really exact and continuous was done "until Sir W. Thomson attacked the problem." He introduced the quadrant electrometer and the water-dropper, which have been the universal recording instruments ever since.

In fact "the work of Palmieri on Mount Vesuvius constitutes perhaps the only extensive series of observations in which instruments founded on the original design of Sir W. Thomson have not been used."

In the States the first energetic and influential mover in the direction of a serious record appears to have been Prof. Cleveland Abbé, who got himself authorised in 1880 by the Chief Signal officer to consult with Prof. Rowland on the subject, and afterwards with Prof. Trowbridge, and to make arrangements for a series of effective observations. Under the auspices of these gentlemen a staff of observers were trained and suitable instruments obtained, tested, and improved. Various collectors were tested, and in 1883 a photographic registration apparatus of M. Mascart was put into operation. In 1884 Mr. Mendenhall "was appointed to assume the direction of the work as chief of the physical laboratory and instrument division of the office in Washington." Stations were established in Washington, Baltimore, Boston, New Haven, Ithaca, and Ohio.

Much work was done in connection with electrometers by McAdie and McRae, but this is incorporated in the article "Electrometer" of the "Ency. Britt."

The instrument ultimately adopted was a quadrant electrometer of the Mascart pattern with special improvements, and was constructed by the Société Génevoise. A picture of it is given.

The method of connecting the quadrants to the two equal halves of a water battery, so that they might always be at equal opposite potentials, and of attaching the needle to the collector, was after many trials adopted; partly because higher insulation was thus possible, partly in order to get a straight line law. Deviation from this, due to what is called the "electric directing couple," is not overlooked, but by a stiff suspension and small range it is minimised.

An interesting chapter is that on "collectors." The water-dropper was mostly used, but its freezing is apt to interrupt the record. "Sergeant Morrill experimented on a special flame collector," supplied with gas at constant pressure and arranged so that wind could not extinguish it, and "before the termination of the work obtained very satisfactory results." But in order to secure uniformity between different stations he also designed a mechanical collector—a clockwork machine with revolving arm and intermittent contacts, which is virtually a gigantic replenisher, utilising the atmospheric potential as an inductor, and thereby feeding the electrometer up to the same potential. It seems to be as quick in response as a water-dropper (an important point, as some of the fluctuations of potential are very rapid), but "as it was only completed towards the end of the period of observation nothing very definite can be said of its performance." An illustration of the ingenious device is given in detail.

Observations.

Preliminary records are given showing the curves got at a roof station and a balcony station, also at different observatories in the same town. Some also from the top of the Washington Monument, which naturally show far greater potential and changes than the instrument in the Signal Office.

There are plotted a number of zigzags obtained from the different stations about the States, and very complicated and entangled the record is. None of the stations show any agreement; and, particularly at Ithaca, the electrometers seem usually to have been in a wildly excited state.

But during an Aurora on May 20, 1888, they were singularly quiet, and the remark is made: "It will be observed that the indications of the electrometer were positive during the day and night, and that no unusual fluctuations occurred."

The atmospheric potential is usually positive, and it has been often thought that a change to negative signalised bad weather. Certainly this does frequently happen; sufficiently often to make it worth while specially to examine this point; and several curve charts are given to show that "negative electricity in clear weather was observed at most if not all of the Signal Service Stations on numerous occasions during the progress of the work. In many cases precipitation occurred at points 10 to 100 miles distant, but in others clear weather prevailed over almost the entire country. A number of instances of negative potential during clear weather occurred at Ithaca, where careful attention was given to the matter of special observation by Mr. Schultze."

Effect of Dust, Haze, Fog.

"The effect of dust, haze, smoke, &c., in producing negative potential has been noticed by more than one observer. [Query whether the negative potential can have ever produced or permitted the haze.—O. J. L.] Several instances of the action of clouds of dust were noted by Sergeant Morrill at Boston. On March 7, 1888, in the afternoon the potential was observed to fall rapidly from − 90 to − 270 upon the rising of an especially heavy cloud of dust, and similar phenomena were observed on April 7." "A fall of potential could be certainly predicted when a dust cloud was seen rising. On other days when high winds and dust clouds prevailed negative potential

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[1] "Report of Studies of Atmospheric Electricity." By T. C. Mendenhall. Extract from Memoirs of the National Academy of Sciences, 1889. (Washington.)